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life this is quite illogical; from baby's milk to grandpa's Glaxo the most important things are organic, excepting water. Food (meat, carbohydrate, fat), clothes (cotton, silk, linen, wool), and shelter (wood) are organic, and the symbols for carbon, hydrogen, oxygen and nitrogen can be made the basis of skeleton representations of many fundamental things which happen to us in our daily lives without first explaining their position in the periodic table of all the elements. The curse of mankind is not labour, but waste; misdirection of time, of material, of opportunity, of humanity.

Realisation of such an ideal would people the ordered communities with a public alive to the verities, as distinct from irrelevancies of life, and apprehensive of the ultimate danger with which civilization is threatened. It would inoculate that public with a germ of the nature-motive, producing a condition which would reflect itself ultimately upon those entrusted with government. It would provide the mental and sympathetic background upon which the future truthseeker must work, long before he is implored by a terrified and despairing people to provide them with food and energy. Finally, it would give an unsuspected meaning and an unimagined grace to a hundred commonplace experiences. The quivering glint of massed bluebells in broken sunshine, the joyous radiance of young beech-leaves against the stately cedar, the perfume of hawthorn in the twilight, the florid majesty of rhododendron, the fragrant simplicity of lilac; periodically gladden the most careless heart and the least reverent spirit; but to the chemist they breathe an added message, the assurance that a new season of refreshment has dawned upon the world, and that those delicate syntheses, into the mystery of which it is his happy privilege to penetrate, once again are working their inimitable miracles in the laboratory of the living organism.

EXPERIMENTAL GEOLOGY

By Dr. J. S. FLETT, F.R.S.

PRESIDENT OF THE GEOLOGICAL SECTION

AMONG the citizens of Edinburgh in the closing years of the eighteenth century there was a brilliant little group of scientific, literary, and philosophical writers. These were the men who founded the Royal Society of Edinburgh in the year 1783, and many of their important papers appear in the early volumes of its Transactions. Among them were Adam Ferguson, the historian and philosopher; Black, the chemist who discovered carbonic acid and the latent heat of water; Hope, who proved the expansion of water on cooling; Clerk of Eldin, who made valuable advances in the theory of naval tactics, and his brother, Sir George Clerk; Hutton, the founder of modern geology;

and Sir James Hall, the experimental geologist. These men were all intimate friends keenly interested in one another's researches. Quite the most notable member of this group was Hutton, who, not mainly for his eminence in geology, but principally for his social gifts, his bonhomie, and his versatility, was regarded as the centre of the circle. Hutton showed an extraordinary combination of qualities. His father was Town Clerk of Edinburgh. After starting as an apprentice to a Writer to the Signet, he took up the study of medicine at the Universities of Edinburgh and Paris, and graduated at Leyden. He then became a farmer on his father's property in Berwickshire, and also carried on chemical manufactures in Leith in partnership with Mr. Davie. He studied methods of agriculture in England and elsewhere, and was an active supporter of the movement for improving Scottish agriculture by introducing the best methods of other countries. A burning enthusiast in geology, especially in the 'theory of the earth,' he travelled extensively in Scotland, England, and on the Continent making geological observations.

His interests were not confined to geology, for he wrote a treatise on metaphysics, which seems to have been more highly esteemed in his day than in ours, and in his last years he produced a work on agriculture which was never published. The manuscript of this work is now in the library of the Edinburgh Geological Society. He also made interesting contributions to meteorology. Hutton's writings are as obscure and involved as his conversation was clear and persuasive, and it is only from the accounts of his friends, and especially Playfair's 'Life of Hutton,' that we can really ascertain what manner of man he was.

It could easily have happened that when Hutton died his unreadable writings might have passed out of notice, to be rediscovered at a subsequent time, when their value could be better appreciated. But Playfair's 'Explanations of the Hutton Theory,' as attractive and convincing still as when it was originally published, established at once the true position of Hutton as one of the founders of geology. Sir James Hall undertook a different task; he determined to put Hutton's theories to the test of experiment, and in so doing he became the virtual founder of modern experimental geology. It is my purpose in this address to show what were the problems that Hall attacked, by what methods he attempted to solve them, and what were his results. I shall also consider how far the progress of science has carried us since Hall's time regarding this department of geological science.

Hutton was a friend of Hall's father: they were proprietors of adjacent estates in the county of Berwick, and much interested in the improved practice of agriculture, and though the elder Hall (Sir John Hall of Dunglass) has apparently left no scientific writings, he was one of those who were famiiliar with Hutton's theories and a member of the social group in which Hutton moved. Sir James Hall was the eldest son; born in 1761, he succeeded to the estate on his father's death in 1776. Educated first at Cambridge and then at Edinburgh University, at an early age he became fascinated by Hutton's personality, though repelled by his theories. He tells us how for three years he argued with Hutton daily, rejecting his principles. Hutton prevailed in the long run, and Sir James Hall was convinced. Hall's objection to Hutton's theories is not difficult to understand, though he has not himself explained it. The world was sick of discussions on cosmogony in which rival theorists appealed to well-known facts as proof of the most extravagant speculations. Serious-minded men were losing interest in these proceedings. The Geological Society of London was founded in 1807, and one of its objects is stated to be the avoidance of speculation and the patient accumulation of facts. No doubt Hall also was greatly influenced by the discoveries that Black and Hope had made by pure experimental investigation. His bent of mind was towards chemical, physical, and experimental work, while Hutton was not only a geologist but also a metaphysician.

Foreign travel was then an essential part of the education of a Scottish gentleman, and the connection between France, Holland, and Scotland was closer than it is today. Hall travelled widely; in his travels two subjects seem to have especially engrossed him. One was architecture, on which he wrote a treatise which was published in 1813 and is now forgotten. The other was geology. He visited the Alps, Italy, and Sicily. In Switzerland he may have met De Saussure and discussed with him the most recent theories of their time regarding metamorphism and the origin of granites, schists, and gneisses. In Italy and Sicily one of his objects was to observe the phenomena of active volcanoes, and to put to the test of facts the theories of Werner and of the Scottish school regarding the origin of basalt, whinstone, trap, and the older volcanic rocks of the earth's crust. At Vesuvius he made his famous observation of the dykes that rise nearly vertically through the crater wall of Somma, which he held to prove the ascent of molten magma from below through fissures to the surface. This was in opposition to the interpretation of the Wernerians, who regarded them as filled from above by aqueous sediments, and Hall's conclusions, which were strikingly novel at the time, have been abundantly confirmed.

We obtain a pleasant glimpse of Hall's life in Berwickshire in the account of his visit with Hutton and Playfair to Siccar Point in the year 1788. The start was made from Dunglass, where probably the party had spent the night. The great conglomerates of the Upper Old Red Sandstone of that district had much impressed Hutton. He saw in them the evidence of new worlds built out of the ruins of the old, with no sign of a beginning and no prospect of an end—a thesis which was one of the corner-stones of his 'Theory of the Earth.' No doubt Hall knew or suspected that in the cliff-exposures at Siccar Point, where the Old Red rests upon the Silurian, there was evidence which would put this dogma to a critical test.

Hall's first experiments were begun in the year 1790, his object being to ascertain whether crystallisation would take place in a molten lava which was allowed to cool slowly. It was generally believed that the results of fusion of rocks and earths were in all cases vitreous, but glassmakers knew that if glass was very slowly cooled, as sometimes happened when a glass furnace burst, the whole mass assumed a stony appearance. An instance of this had come under Hall's notice in a glassworks in Leith, and its application to geology was clear. Hutton taught that even such highly crystalline rocks as granite had been completely fused at the time of their injection, and their coarse crystallisation was mainly due to slow cooling.

For the purpose of his experiments Hall selected certain whinstones of the neighborhood of Edinburgh, such as the dolerites of the Dean, Salisbury Crags, Edinburgh Castle, the summit of Arthur's Seat, and Duddingston; but he also used lava from Vesuvius, Etna, and Iceland. He made choice of graphite crucibles, and conducted his experiments in the reverberatory furnace of an ironfoundry belonging to Mr. Barker. As had been shown by Spallanzani, to whose experiments Hall does not refer, lavas are easily fusible under these conditions. Hall had no difficulty in melting the whinstones and obtaining completely glassy products by rapid cooling. He now proceeded to crystallise the glass by melting it again, transferring it from the furnace to a large open fire, where it was kept surrounded by burning coals for many hours, and thereafter very slowly cooled by allowing the fire to die out. He succeeded in obtaining a stony mass in which crystals of felspar and other minerals could be clearly seen. Some of his specimens were considered to be very similar in appearance to the dolerites on which his experiments were made.

The only means of measuring furnace temperatures available at that time were the pyrometers which had recently been invented by Wedgwood. Hall found that a temperature of 28 to 30 Wedgwood yielded satisfactory results. This seems to be about the melting-point of copper, approximately 1000° C.

Whether by design or accident, Hall chose for his experiments precisely the rocks which were most suitable for his purpose. If granite had been selected no definite results would have been obtained. De Saussure had already made fusion experiments on granite. Ninety years afterwards the problem was completely solved by Fouqué and Lévy, who used a gas furnace and a nitrogen thermometer. They found that it was possible to obtain either porphyritic or ophitic structure by modifying the conditions, and that the minerals had exactly the characters of those of the igneous rocks. Some of Hall's recrystallised dolerites were examined microscopically by Fouqué and Lévy, and, as might be expected, they proved to be only partly crystallised, showing skeleton crystals of olivine and felspar with grains of iron ore in a glassy base.

Some curious observations made by Hall in his experimental work were also confirmed by Fouqué and Lévy. The crystalline whinstones were more difficult to melt than the glasses which were obtained from them, and the glass crystallised best when kept for a time at a temperature a little above its softening point. It is not possible to assign a definite melting-point to the Scottish whinstones with which Hall worked. Many of them contain zeolites, which fuse readily. Minerals are also present that decompose on heating, such as calcite, dolomite, chlorite, and serpentine. The whole process is very complex, and probably takes place by several stages not sharply distinct. Similarly the glasses cannot be said to have a melting-point. They are really super-cooled liquids. A full explanation of what took place in Hall's crucibles cannot be given at the present day, but there is no room for doubt that his experiments were good and his inferences accurate. His friend Kennedy, who had recently discovered the presence of alkalis in igneous rocks, furnished valuable support to Hall's conclusions by showing that the chemical composition of whinstone and of basalt were substantially identical.

Apparently the results of Hall's work were not received with unmixed approbation. Hutton was distinctly uneasy, and it has been suggested that he feared if experimental work turned out unsuccessful it might bring his theories into discredit. The Wernerians frankly scoffed; they preferred argument to experiment, and the endless discussion went on. Gregory Watt repeated Hall's experiments by fusing Clee Hill dolerite, a hundredweight or two at a time, in a blast-furnace. But there can be no doubt that among those who were not already committed to the principles of Werner the new evidence produced a strong impression, and helped to widen the circle of Hutton's supporters.

Hall's most famous experiments were on the effect of heat combined with pressure on carbonate of lime. The problem was, Can powdered chalk be converted into firm limestone or into marble by heating it in a confined space? In this case Hutton's theories were in apparent conflict with experimental facts; from general observations he held it proved that heat and pressure had consolidated limestones and converted them into marbles. It was well known, of course, that limestone, when heated in an open vessel, was transformed into quicklime, and Black had shown that the explanation was that carbonic acid had been expelled in the form of gas.

The experiments were begun in 1790, but deferred till 1798 after

Hutton's death. Hutton quite openly disapproved of experiments. His famous apophthegm has often been quoted about those who 'judge of the great operations of the mineral kingdom by kindling a fire and looking in the bottom of a crucible.' In deference to the feelings of his master and his father's friend, Sir James Hall, with admirable self-restraint, decided not to undertake experimental investigations in opposition to Hutton's expressed opinion. With a few month's interruption in 1800 they were continued till 1805. A preliminary account of the results was communicated to the Royal Society of Edinburgh on August 30, 1804, and the final papers submitted on June 3, 1805. Hall states that he made over 500 individual experiments and destroyed vast numbers of gun-barrels in this research.

The method adopted was to use a muffle-furnace burning coal or coke and built of brick. No blast seems to have been employed. The chalk-powder was enclosed in a gun-barrel cut off near the touch-hole and welded into a firm mass of iron. The other end of the barrel could be kept cool by applying wet cloths, and as it was not in the furnace its temperature was always comparatively low. Various methods of plugging the barrel were adopted; at first he used clay, sometimes with powdered flint. Subsequently a fusible metal which melted at a temperature below that of boiling water was almost always preferred. Borax glass with sand was used in some of the experiments, but it was liable to cracking when allowed to cool, and consequently was not always gas-tight. It was essential, of course, that in sealing up the gun-barrel, and in subsequently removing the plug, the temperatures should never be so high as to have any sensible effect on the powdered chalk or limestone. Hall tried vessels with screwed stoppers or lids at first, but never found them satisfactory.

In the gun-barrel there was always a certain amount of air enclosed with the chalk. Very early in the experiments it was shown that if no air-space was provided the fusible metal burst the barrel. No means was found to measure the size of the air-space accurately, but approximately it was equal to that of the powdered chalk used in the experiment. If the air-space was too large, or if there was an escape of gas, part of the chalk was converted into lime.

As each experiment lasted several hours the temperature of the chalk was approximately equal to that of the part of the muffle in which it was placed. Pyrometry was as yet in its infancy. Wedgwood had invented pyrometric cones and Hall had heard of them, but apparently at first he was not in possession of a set. He made his own cones as nearly similar as possible to those of Wedgwood, and subsequently obtaining a set of Wedgwood's cones he standardized his own by comparison with them. His gun-barrels of Swedish and Russian iron ('Old Sable') were softened, but seldom gave way except when the internal pressures were of a high order. Some of the gun-barrels seem to have been used for many experiments without failure occurring. As Hall made his own pyrometric cones, and we have no details of their composition and the method of preparation, it is not possible to do more than guess at the temperatures to which his powdered lime and chalk were exposed. There is no doubt that by constant practice and careful observation he was able to regulate the temperature within fairly wide limits.

Hall began his experiments as already stated in 1798. They were interrupted for about a year (March 1800 to March 1801), and on March 31, 1801, he had obtained a considerable measure of success. A charge of forty grains of powdered chalk was converted into a firm granular crystalline mass of limestone. The loss on weighing was approximately 10 per cent. Another charge of eighty grains was converted into marble (on March 3, 1801), with a loss of approximately 5 per cent., and the crystalline mass showed distinct rhombohedral cleavage.

Though it cannot be said that his success was easily won he was by no means satisfied, and for another four years he continued his researches. Many different methods were tried in order to ascertain the most satisfactory and reliable; his ambition was to attain complete control of the process so that he could always be certain of the result. Porcelain tubes were tried, which he obtained from Wedgwood. They were very liable, however, to allow escape of the gases through pores. Many different methods of obtaining gas-tight stoppers were experimented on, but he does not seem to have found anything really better than the fusible metal. A slight loss of weight in the chalk used seemed inevitable, and the amount of loss varied irregularly; after long trials he ultimately succeeded in reducing this to less than one per cent. Various kinds of carbonate of lime were used, including chalk, limestone, powdered spar, oyster shells, periwinkles, and each of these was crystallised in turn. Many experiments showed that a reaction might take place between the chalk powder and the glass of the tube in which it was contained. The result was a white deposit often crystalline, and a certain amount of uncombined carbonic acid gas which escaped when the tube was opened. No doubt the white mineral was wollastonite. Hall proved that it was a silicate of lime which dissolved in acid and left a cloud of gelatinous silica. Thereafter he used platinum vessels instead of glass to contain the charge of carbonate of lime which he wanted to fuse. The effect of impurities in the material used was also investigated. Critics had urged that his limestone was not pure. Hall aptly replied that this was so much the better; natural limestones were seldom pure, and his point was that limestone might be fused under heat and pressure. He obtained the purest precipitated carbonate of lime, and used also perfectly transparent crystalline spar; the results were, as we might expect, that the pure substances and the fairly coarse crystalline powder were more difficult to fuse than the very finely ground natural chalk. These results show that Hall had very complete control of his experimental processes, and that even small differences in fusibility did not escape his observation.

As natural limestones are always moist, Hall's attention was next directed to the influence of water on the crystallisation of his powders. This added greatly to the difficulty of the experiments, but by wonderful skill he succeeded in using a few grains of water (apparently up to five per cent. of the weight of the chalk). The result was to improve the crystallisation, for the reason, as Hall believed, that the pressure was increased. He noticed at the same time that hydrogen was produced, which took fire when the gun-barrel was discharged. Probably there was also some carbonic oxide. About this time he was using bars of Russian iron into which a long cylindrical cavity had been bored. He then tried other volatile ingredients such as nitrate of ammona, carbonate of ammonia, and gunpowder. In January 1804 he was able to convert chalk into firm limestone at a temperature about 960° (melting-point of silver) in presence of small quantities of water with a loss of less than one-thousandth part of the chalk used.

Finally he attempted to measure the pressure which was necessary to effect re-crystallisation under the conditions of his experiments. No pressure gauges were available at that date, and after many trials he employed a stopper faced with leather and forced against the mouth of his iron tube by means of weights acting either directly or through a lever. He ultimately succeeded in obtaining gas-tight junctions under pressures ranging from 52 up to 270 atmospheres, and concluded that 52 atmospheres was the least pressure which could be satisfactory. This is equal to the pressure of a column of water 1,700 feet high or to a column of rock 700 feet high. A 'complete marble' was formed at a pressure of 86 atmospheres and carbonate of lime 'absolutely fused' under a pressure of 173 atmospheres.

In reviewing these classic experiments after a lapse of 120 years we feel that there are many points on which we should have liked more detailed information. One essential, for example, is exact chemical analysis of all the materials employed. Even chalk is variable in composition to a by no means negligible extent. Oyster shells and periwinkle shells contain organic matter, which would account for the considerable loss in weight they always exhibited. The use of glass tubes was a defect in the early experiments afterwards remedied by employing platinum vessels. Although in all the experiments the charge was weighed it seems clear that at first at any rate the materials were not carefully dried. In the experiments with water it was seldom possible to provide absolutely against the escape of moisture when the fusible metal was introduced. Most of all we may regret the inadequate means of measuring the temperatures at which the experiments were conducted. The measurements of pressure were made by the simplest possible means, and it was only by great experimental skill and care that even approximate results could be obtained.

Such criticisms, however, do not mar the magnificent success of Hall's experiments. For nearly a hundred years, in spite of the advance of physical and chemical science, no substantial improvement on his results was attained. His work was immediately recognized as trustworthy and conclusive, and became a classic in the literature of experimental geology. Although not exactly the founder of this school of research, for Spallanzani and De Saussure had made fusion experiments on rocks before his time, he placed the subject in a prominent position among the departments of geological investigation, and did great service in supporting Hutton's theories by evidence of a new and unexpected character.

SOME PROBLEMS IN EVOLUTION

By Professor EDWIN S. GOODRICH, F.R.S.
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IN all probability factors of inheritance exist, and the fundamental problem of biology is how are the factors of an organism changed, or how does it acquire new factors? In spite of its vast importance, it must be confessed that little advance has been made towards the solution of this problem since the time of Darwin, who considered that variation must ultimately be due to the action of the environment. This conclusion is inevitable, since any closed system will reach a state of equilibrium and continue unchanged, unless affected from without. To say that mutations are due to the mixture or reshuffling of pre-existing factors is merely to push the problem a step farther back, for we must still account for their origin and diversity. The same objection applies to the suggestion that the complex of factors alters by the loss of certain of them. To account for the progressive change in the course of evolution of the factors of inheritance and for the building up of the complex it must be supposed that from time to time new factors have been added; it must further be supposed that new substances have entered into the cycle of metabolism, and have been permanently incorporated as self-propagating ingredients entering into lasting relation with pre-existing factors. We are well aware that living protoplasm contains molecules of large size and extraordinary complexity, and that it may be urged that by their combination in different ways, or by the mere regrouping of the atoms within them, an almost infinite number of changes may result, more than sufficient to account for the mutations which appear. But this does not account for the building up of the original complex. If it must